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THE JOURNAL OF PHILOSOPHY

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VOL. XLIX, NO. 24: NOVEMBER 20, 1952

CONTENTS

The Principle of Induction: ASHER MOORE 741

An Analytic Principle of Induction?: MAY BRODBECK 747

The Principle of Induction (II): A Rejoinder to Miss Brodbeck: ASHER MOORE 750

Book Reviews

JUSTUS BUCHLER, *Toward a General Theory of Human Judgment:* MASON W. GROSS 758

C. D. BROAD, *Ethics and the History of Philosophy; Selected Essays:* GEORGE BOAS 762

New Books 765

Notes and News 768

PUBLISHED FORTNIGHTLY

BY THE

JOURNAL OF PHILOSOPHY, INC.

THE JOURNAL OF PHILOSOPHY

FOUNDED BY FREDERICK J. E. WOODBRIDGE AND WENDELL T. BUSH

EDITORS: Herbert W. Schneider, John H. Randall, Jr., Ernest Nagel.

MANAGING EDITOR: Emerson Buchanan.

Communications to the editors should be addressed to the Managing Editor, 515 West 116th Street, New York 27, N. Y. Subscriptions and advertisements should be sent to THE JOURNAL OF PHILOSOPHY, Prince and Lemon Streets, Lancaster, Pa., or 515 West 116th Street, New York 27, N. Y. The annual subcription price (twenty-six numbers) is $5.25. The price of single numbers is 35 cents, double numbers, 60 cents.

THE JOURNAL OF PHILOSOPHY is owned by the Journal of Philosophy, Inc., and published fortnightly at Prince and Lemon Sts., Lancaster, Pa. Editorial Office: 515 West 116th St., New York 27, N. Y. President, Ernest Nagel; Vice President, Herbert W. Schneider; Treasurer, Frederick A. Dewey; Secretary, Iris Wilder Dean.

Entered as second class matter, March 18, 1921, at the Post Office at Lancaster, Pa., under Act of Congress of March 3, 1879.

Acceptance for mailing at special rate of postage provided for in paragraph (d-2), section 34.40, P. L. & R. of 1948, authorized March 11, 1952.

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VOLUME XLIX, No. 24 NOVEMBER 20, 1952

THE JOURNAL OF PHILOSOPHY

THE PRINCIPLE OF INDUCTION[1]

FOR the purpose of this paper I shall not, fortunately, have to try to state the Principle of Induction with any great precision. The following will do: it is more probable than not that uniformities, either universal or statistical, which have been observed to hold uniformly in past experience will continue to hold in the future.

Hume thought there could be no evidence of any sort for the Principle of Induction. It is not, he argued, analytic, since there is nothing in the idea of past uniformities which implies that these uniformities must continue. It is not self-contradictory to assert that any or all uniformities will cease to hold, beginning with the next case. Nor can the Principle of Induction be evidenced by appeal to past experience, since any such appeal is bound to be circular. And these are the only two possible sorts of evidence for any proposition.

I wish to suggest that Hume was mistaken in thinking that the Principle is not analytic.

It is of course true, as he claimed, that no assertion about past uniformities analytically implies the continuance of these uniformities. But this is beside the point. For the Principle of Induction does not assert a connection between past facts and future facts. What it asserts is a connection between past facts and the *probability* of certain future facts—that is, between past facts and *what it is reasonable to believe* about future facts. And *this* connection, I suggest, is analytic. It is analytic of the concept of probable, or reasonable.

The issue hinges, of course, upon what is meant by such terms as probable, reasonable, and their synonyms. We are accustomed to regard a probability statement as being predictive, and we distinguish between the future fact predicted by the statement and the past facts which are taken to be its evidence. This way of looking at the matter makes the connection between past facts and probability statements synthetic and makes Hume's skepticism irrefutable. In order to make my point that the connection is really analytic I shall have to show that probability statements are really non-predictive. I shall have to show that they are analytically

[1] Read at the meeting of the Western Division of the American Philosophical Association at the University of Michigan, May 9, 1952.

derivable from statements describing past facts and are, indeed, simply alternative ways of describing certain characteristics of those facts.

Consider any predictive statement which says that a certain event, d, will have a certain characteristic ϕ. I shall call this statement sometimes 'P', sometimes, when I want to distinguish its components, 'ϕd'. What does it mean to say that, on certain data, P has a probability of, say, a/b?

Both Lewis and Carnap have distinguished two fundamentally different senses of the term probable. Carnap calls the first of these 'probability$_2$' or 'relative frequency in the long run'. By Lewis this sense is called either 'the expectation coefficient' or else simply 'probability'. In this sense of 'probable' our statement to the effect that ϕd has a probability of a/b is really a statement about a frequency. Roughly, what it says is that d has some reference characteristic ψ and that the frequency of ϕ's among the ψ's is a/b.

Carnap believes this frequency statement to be an empirical judgment, actually predicting the frequency of ϕ's among ψ's which will occur in the long run, and subject to confirmation and disconfirmation by observation. Lewis, on the other hand, argues that the frequency statement which is equivalent to our original probability statement is not the empirical statement 'The frequency of ϕ's among the ψ's actually is a/b' but rather the statement 'The frequency of ϕ's among the ψ's is, on data D, validly estimated to be a/b.' And he holds that this latter statement is, given the data upon which it is based and the rules of probability inference, analytic.

I need not decide this disagreement. For it is clear that it is not this "frequency" sense of 'probability' which is involved in the problem of induction. That it is not can be shown on either Carnap's or Lewis' view.

Adopting Carnap's view that probability$_2$ statements are to be translated into *predictive* frequency statements, then it is apparent that no such statement can be more than partially credible. We can only estimate, on the basis of past experience, what the frequency is. The most we can say is: "On the basis of past experience, it is *probable* to some degree that the frequency estimate a/b is correct." And it is *this* sense of 'probable', not the frequency sense, which is involved in the problem of induction. Carnap calls this second sense 'probability$_1$' or 'degree of confirmation'.

Or take Lewis' view that the statement 'On data D, P has probability a/b' is to be translated into the analytically true statement 'On the data, the frequency of ϕ's among the ψ's is validly estimated to be a/b.' The question then immediately arises as to how good

a "valid estimate" this is. Given the data, the estimate is valid, to be sure, provided only that one has followed the correct rules of inference; but how good are the data upon which the estimate is made? For example, we should want to say that, other things being equal, a valid estimate based on a large sample is a better estimate than one based on a smaller sample. Lewis calls this factor of "how good is your valid estimate?" the 'reliability' of the estimate.

Once again, it is clear that it is this factor of reliability, not the factor of probability, or "expectation coefficient," that is involved in the problem of induction. For the problem of induction concerns whether any estimate of future events made on the basis of past experience has more than 0 reliability.

From this point on, therefore, I shall mean by 'probable' what Lewis means by 'reliable' and what Carnap means by 'degree of confirmation'. The statement 'P is probable', as now interpreted, need not say anything, either analytic or predictive, about frequencies. It says simply that past experience supports P, or makes it reasonable to believe, in some degree.

I now return to the original question: What does it mean to say that P has a probability of a/b?—taking the term 'probability' in the sense I have just distinguished.

Now it might be claimed that this statement is, even in this new sense of 'probable', an empirical, predictive statement of a frequency. One might want to suggest, for example, that it predicts, not now the frequency of ϕ's among ψ's, but the frequency of true sentences in a "long run" of sentences having the same sort of relation to past experience that P has. So interpreted, the statement would be taken as predicting that a/b of these sentences are true.

But, if we are ever going to be able to make any plausible claim to know any probabilities at all, we cannot interpret *all* probability statements as being predictive, either in this way or in any other way. As Lewis has shown in a somewhat different context and perhaps somewhat inexactly, to do so would involve us in an infinite regress. For no one would want to claim that he knows the predicted facts with certainty. The only plausible claim would be that we know what they *probably* are. That is, the claim would be, at best, that we know a probability. But, on this theory, to know a probability is to predict correctly some future facts. And these facts in turn could be known only with probability—that is, in terms of our knowledge of still further future facts, themselves knowable only with probability, and so on and on. To claim to know a probability would, on this interpretation, involve an infinite series of claims.

Consequently, if we are to be able to make any plausible claim

to know any probability at all, we must somewhere stop interpreting probability statements as asserting facts of a sort which can be known only with probability, and start interpreting them as asserting something which is knowable with certainty on the basis of the data already available. And if we are going to have to do this sooner or later, we might as well do it sooner. We might as well hold that our very first probability statement—'On data D, P has probability a/b'—is not predictive, but is the sort of statement the truth of which follows from the data.

But this means that a probability statement is, given the data, analytic. Insofar as it asserts the data ('On data D') it has empirical content; but all it asserts in addition to the data is a probability which is analytically derivable from the data by the rules of evidence. It does not predict anything, and hence no future experience can be relevant to its truth. If correctly inferred, it is true regardless of what the facts turn out to be. The statement 'Given data D, then P has probability a/b' is in all relevant respects like the statement 'Given that I have 2 plus 2 pieces of chalk in my hand, then I have 3 plus 1 pieces of chalk in my hand.'

But how do we know what rules of evidence will give us a correct probability judgment, or, indeed, that any rules will? Well, simply by knowing what we mean by the term probable. To say that, on data D, P has a certain probability is simply to say that the data D have certain characteristics, and we know what these characteristics are by knowing what we are saying in calling P probable.

And it is, in a very rough way, obvious *what* characteristics of the data are being referred to when we say that a frequency estimate (whether universal or statistical) based on those data is relatively more probable than one based on some other data. Roughly, we are saying that the former set of data is larger, that there is less reason to think it unrepresentative of the total universe, and that it is more homogeneous. And to say that, on the basis of the totality of past experience, a frequency estimate is highly probable is to say that large quantities of past experience, not known to be biased, have uniformly shown this frequency.

What more precisely these relevant characteristics of past experience are, and how they function in metrical and comparative judgments of probability, is a subtle and difficult matter. Much important work is being done in an effort to determine them more exactly. This paper does not pretend to make any contribution to these detailed studies. The only point I am making is that probability judgments *do* refer to *some* characteristics of past experience; that they are shorthand ways of describing these characteristics.

Now the Principle of Induction is, precisely, a probability statement. It says 'Given past uniformities, it is more probable than not that these uniformities will continue.' This is accordingly a statement which follows analytically from the past uniformities. To say that it is highly probable that a certain specific uniformity will continue is to say that this uniformity has been uniformly observed to hold in the relevant parts of past experience. And to say that, in general, future experience as a whole will most probably manifest a certain degree of uniformity is to say that past experience always has, uniformly, day after day, over a long period of time, manifested this degree of uniformity.

The Principle of Induction is therefore true of all possible worlds where there are any past uniformities to apply it to (and it is vacuously true of other worlds). In any such world, and whatever may actually turn out to be the future disuniformity of it, it is probable (i.e., reasonable to believe) that past uniformities will continue. (That means: past uniformities have continued.) And I do not think this reasonableness can be upset by any talk about omniscient devils who may suddenly decide to start upsetting all of our predictions. You cannot show that a belief is unreasonable by showing that it might turn out to be false. Of course it might. But the question is whether it is reasonable to think that it will. It is not self-contradictory to say that although all of past experience indicates that loaves of bread do not have teeth, the next loaf may bite off my tongue. About this Hume was right. But it *is* self-contradictory to say that, given past experience as it has been, the *probability* that the next loaf will bite is just as great as the probability that it won't.

Since the Principle of Induction is analytically true, there is no need for any metaphysical postulate of the uniformity of nature such as many philosophers have thought necessary. Nor can I see how such a postulate would be at all possible. If the Principle of Induction be phrased in terms of the *probable* continuance of uniformities, as I have phrased it, then it is a non-factual statement and could not possibly be a metaphysical assertion about nature. Consequently, if we wanted to put it forth as a metaphysical statement, we should have to phrase it "It is certain that past uniformities will continue." And I cannot imagine that anyone would seriously maintain such an assertion.

I imagine you now feel that I have missed the point. Usually, the Principle of Induction is used to validate inferences from past experience to the probability of some predictive statement like P. But I have not said anything about P since the very beginning of my paper. What I have been talking about is the very different

proposition 'P is probable.' I have said that the statement 'P is probable' is, given the data, analytic; that it is, indeed, a statement about the data. Doesn't this still leave us without any assurance about P? Usually, we would say that the problem of induction is to find a connection between past facts and predictive statements. Haven't I simply translated the problem into other terms, leaving myself with the problem of finding a connection between 'P is probable' (which, according to me, translates into 'the past facts have been such-and-such') and 'P' (which predicts a future event)?

But it seems to me that I *have* given assurance with respect to the predictive statement, P. I have told you that, given the data as they have been, P is probable. To be sure, 'P' and 'P is probable' are not the same proposition. One is predictive; the other is analytic of past data. But surely the statement 'P is probable' *does* tell us that the predictive statement, P, is probable; that it is reasonable; something that ought to be believed by a rational person.

If you still feel that I haven't given any assurance about P (but instead simply an analytic statement deducing something from past data), then I would like to know what sort of assurance you would like. What would you like to know about P? Not, surely, that it is certainly true. Then what? That what P asserts will actually happen? that the chances will mature? But what does that mean? That the chances will *certainly* mature? Such assurance should not be demanded. That the chances will probably mature? That assurance I have already given.

I feel that philosophers who see an unsolved problem here are really thinking in terms of an illegitimate 3-valued scale. The values are: (1) Is P certainly true? (2) Is P probably true? (3) Is P true? (i.e., will the probability "come true"?). Having admitted that P is not certainly true, and having been shown that P is probably true, they want to go on to ask—but *is* P true? But this seems to me foolish. The answer is always the same: what do you want to know? whether P is certainly true, or whether it is probably true?

I presume you are now thinking that all of this is a pretty bald tautology. I agree with you. I have "solved" the problem of induction by definition. I have said: "Yes, it *is* reasonable to believe that past uniformities will continue, because to say that this is reasonable is to say that it has always been so."

As I said earlier, the issue hangs upon what is meant by 'probable' or 'reasonable'. I have presented a sense of 'probable' which seems to me to accord with ordinary usage and from which I think the Principle of Induction follows analytically. Is there some other

relevant sense of 'probable' in terms of which we could raise a problem about whether the continuance of past uniformities is probable? I cannot see any, except one which might be manufactured just to create problems.

ASHER MOORE

NORTHWESTERN UNIVERSITY

AN ANALYTIC PRINCIPLE OF INDUCTION?[1]

MR. Moore is so forceful and so disarming that I confess on first reading I found it hard to withstand the persuasive power of his "what else do you want to know?" What else, indeed? Almost I was persuaded. But—perhaps unfortunately—I collected my wits and reread the paper. The second time I found that my disagreements with Mr. Moore are both wide and deep. The traditional problem of induction lies in the attempt to "justify" the interesting habit men have of making inferences from the observed to the unobserved, from the past to the future, or, more generally, from the known to the unknown. The results of such inferences are formulated either as universal laws or as statistical probability statements, that is, statements about the limits of frequencies. Both types are generalizations, going beyond our observations, and are, therefore, inductive, hypothetical, or, as we say, merely probable. A classical way of justifying this procedure is to formulate a principle of induction which is supposed to validate all specific instances of induction. Mr. Moore avoids the notorious difficulties of this enterprise by presenting us with an analytic truth and trying to persuade us that this analytic truth is all that is meant or, at least, could be reasonably meant by the proponents of a "Principle of Induction." This takes some courage, since it is not easy to see what purpose is served by interpreting the supposed principle in this manner. For, as Mr. Moore would agree, nothing follows from a tautology. What, then, does it justify? In fact, Mr. Moore makes the principle analytic by interpreting 'probable' as synonymous with 'reasonable to believe' and the latter, again, as synonymous with the *evidence* on which what is ordinarily called induction, generally or in special cases, is based. This, as I shall try to show, is too high a price to pay. Or, to put it differently, if he has succeeded in justifying anything, this thing is no longer worth justifying.

[1] Read, in discussion of the preceding paper, at the annual meeting of the Western Division of the American Philosophical Association at the University of Michigan, May 9, 1952.

Mr. Moore's procedure raises two issues: First, he believes the Principle of Induction to be strictly analogous to other so-called probability statements or, less ambiguously, to special instances of induction. The issue here is as to the legitimacy of the analogy. Secondly, since he argues by analogy, the correctness of his analysis of what he calls "probability statements" is also at stake. What, then, can be said about his analysis of probability statements?

Mr. Moore correctly points out that the connotation of 'probable' which, if any, is connected with induction is *degree of confirmation* or *reliability*, not limit of a frequency, since statements about the latter are themselves inductions. However, one must wonder if he takes sufficiently into account the force of the distinction between these two meanings of 'probability'. For he believes that unless we start with some probability known, on the evidence, for certain, we are involved in an infinite regress. If the distinction is kept in mind, there is clearly no infinite regress; in fact, no regress at all. Consider the proposition 'It is probable that the probability of getting heads is ½.' In the dependent clause, 'probability' refers, of course, to the limit of a frequency. The first use of 'probability', on the other hand, is identical with that in 'It is probable that we will have chicken and peas at the banquet tonight.' This 'probable' is a locution for 'merely inductive', 'hypothetical', 'based on generalization', or some such phrase. The full sentence cannot itself be said to be probable; rather, it is a statement of philosophical analysis clarifying the nature of our knowledge of any nonanalytic general statement. The belief Mr. Moore shares with Lewis and perhaps others that there is an infinite regress involved in statements about frequencies, whether these be frequencies of truth values or of physical properties, is based purely on an equivocation. Moore nevertheless concludes from this alleged regress that 'probability' statements must express something that can be known with certainty or, more precisely, known thus on the basis of data about the past. So he is led to believe that his paradigm, 'On data D, P has probability a/b', is analytic.

At this point a certain ambiguity pervades Mr. Moore's discussion. If the "probability statement" to be analyzed is the whole conditional, 'On data D, P has probability a/b', and not, as one might expect, only the consequent, then, to be sure, this statement may be construed as analytic, if by 'probability' one means 'degree of confirmation' or some synonym thereof. For 'degree of confirmation', as used by its proponents, names indeed a logical relation between evidence and hypothesis. Upon this interpretation, the paradigm has, as is well known, no empirical content whatsoever. Mr. Moore agrees with this. The consequent (or hypothesis), in his

variant of 'degree of confirmation', merely repeats the antecedent (or evidence); therefore, he concludes, to say 'P is probable' is merely to assert that Data D are such and such and to make no statement whatever about anything that goes beyond them. Suppose that P is the proposition that cats meow. According to Mr. Moore, to say that P is probable is to say that all observed cats have meowed. Similarly, suppose that P is the proposition that the limit of the frequency of heads in tosses of an unbiased coin is ½. According to Mr. Moore, to say that P is probable is to say, roughly, that all such observed frequencies have been ½. But is not the problem of induction precisely the problem of the justification for saying more than what has been observed, for asserting P itself? Yet, Mr. Moore believes that he has told us something about P, namely that it is probable. All he has done is give us a definition "in use" of the whole sentence 'P is probable' to mean 'Data D'. This, I submit, has nothing to do with what is meant by the problem of induction. Some believe, indeed, that there is no general problem of induction or, more precisely, that what is asked is not a good question. One could agree with this view and yet insist that whatever problem there is must not be ruled out of court in this manner.

Furthermore, Mr. Moore himself is involved in an infinite regress and, I think, a vicious one. Sometimes he speaks as if—and, I think, he means—the antecedent of his tautological sentence does not really read 'On data D' but, more fully, 'Given data D and certain rules of inference'. I presume these rules tell him also why a larger set of data is preferable to a smaller one; why one sample is more representative than another; why more homogeneous. Yet, all such knowledge is itself based on induction. This, I believe, is an important difference between Carnap's degree of confirmation and Lewis's reliability of a valid estimate. Mr. Moore's view, it seems to me, is closest to Lewis's. As I understand it, Carnap's rules are based on mathematical, combinatorial possibilities in a formalized language, while Lewis, like Moore, has in mind certain rule-of-thumb or empirical procedural rules. What Carnap gains in precision he loses of course in relevance, while Lewis and Moore are left with a whole set of inductions to justify. I am not saying that there is anything wrong with this. I think, in fact, that it is unavoidable, being of the nature of the scientific enterprise. I am only pointing out that Mr. Moore is still left with the problem of induction. He has merely pushed it back one step, as it were, into the antecedent of his tautological conditional.

After all this, I shall be brief about the analogy between specific inductions and the so-called Principle of Induction. In analogy with 'On data D, P is probable', Mr. Moore construes the Principle

of Induction to assert 'Given past uniformities, it is probable they will continue.' As the first is analytic, so, we are told, is the second. It follows that in the Inductive Principle too 'probability' means 'degree of confirmation'. There are two things to be said about this analogy. First, while in the case of a specific reliability sentence, relating evidence to hypothesis, we cannot say anything about either the evidence or the hypothesis, we can perhaps by means of the rules say something about the logical relation between them, namely, that the data D together with the rules confirm P to a certain degree. For the generalized Inductive Principle, on the other hand, 'probable' means no such relatively precise concept but, in Moore's terms, what it is rational to believe. Naturally not, since as we saw, the rules are part of the material content of the principle. Yet Mr. Moore concludes, by identifying 'rational-to-believe' with his Lewis-like variant of 'degree of confirmation', that the consequent of his inductive principle merely repeats the antecedent and it alone. It follows, then, and this is my second point about the analogy, that the Principle of Induction itself is merely a statement about the past. This is no doubt quite consistent within his analysis. But the price he pays is high indeed. For we see now that upon his analysis of the word 'probability' the inductive principle reads not 'If uniformities in the past, then probably the same uniformities in the future' but, instead, 'If uniformities in the past, then uniformities in the past.' On this we could agree. Yet if there is any problem of induction, it is about the relationship between the observed and the unobserved; merely to repeat that the observed facts are what they are leaves the mystery, if there is one, right where it was.

MAY BRODBECK

UNIVERSITY OF MINNESOTA

THE PRINCIPLE OF INDUCTION (II): A REJOINDER TO MISS BRODBECK

I SHALL comment briefly on two of Miss Brodbeck's points which seem to me of subsidiary importance. I shall then discuss more fully her more crucial objections to my view that the Principle of Induction is analytic.

(1) I asserted that if we are to be able to make any plausible claim to know any degree-of-confirmation statements then we must construe such statements to be the sorts of things that can be known with certainty on the basis of available data. That is, we must construe them to be, given the data, analytic. My argument, which

roughly followed Lewis, was that otherwise we are involved in an infinite regress. Unless we know with *certainty* that P has degree of confirmation a/b, then either we don't know it at all, or else we know simply that the statement 'P has degree of confirmation a/b' has itself a certain degree of confirmation c/d. And unless we know this statement with certainty, then either we don't know it at all, or else we know simply that it has in turn a degree of confirmation e/f. And so on. Thus the alternatives are: (a) to be complete skeptics about all degree-of-confirmation statements; (b) to admit that in knowing one such statement we know an infinite number of them, which is to claim the impossible; or (c) to confess that, on the basis of available data, we can know degree-of-confirmation statements with certainty. To adopt (c) is to hold that, given the data, degree-of-confirmation statements are analytic.

Miss Brodbeck says this argument is fallacious, a result of my having confused degree-of-confirmation with limit-of-a-frequency. I think I have not made this confusion; the argument still seems to me sound. I shall not develop it in detail, since anyone who is interested may follow it in Lewis' *An Analysis of Knowledge and Valuation*. My argument differs from Lewis' in only two ways. He uses it in connection with a demonstration that judgments of expectation coefficients are analytic, whereas I use it in a demonstration that reliability judgments are analytic. I do this because I cannot at the moment see why judgments of expectation coefficients should not be treated as empirical statements (in the manner of Carnap), *provided* reliability judgments are held to be analytic. Secondly, Lewis claims that the regress occurs in construing the *meaning* of probability judgments in accordance with an empirical interpretation. I cannot see any regress at this stage. The empiricist can simply say that, according to him, 'P is probable' means that the limit of a frequency is such and such. There is no regress involved in this. The regress occurs when we ask him how he *knows* that this is what the limit is. When asked this question, he must either be a total skeptic (i.e., make no claim to knowledge), assert that he knows the empirical frequency limit with certainty (i.e., make an implausible claim to knowledge), or else involve himself in an infinite series of claims (that is, claim that in knowing a degree-of-confirmation statement what he knows is its degree-of-confirmation, and so on *ad infinitum*). Except for these two points, I think Lewis' argument is correct.

Although Miss Brodbeck claims to disagree with me on this matter, I am not sure she really does. For she herself allows that the statement 'P has degree-of-confirmation a/b' is a "statement of philosophical analysis." And I imagine she would want to say that

statements of philosophical analysis are analytic—which is what I am saying.

(2) Miss Brodbeck claims that I am myself involved in an infinite regress of a vicious sort. For she thinks that my 'On Data D' in the statement 'On Data D, P has probability a/b' ought to be expanded into 'On Data D plus the rules of induction'. But since the rules of induction (i.e., such rules as that large samples are better than smaller ones) must themselves be established inductively, she argues that I have simply pushed the problem of induction back into the phrase 'On Data D'.

This point can be quickly cleared up. I do not believe that the rules of induction must themselves be justified by induction, for I do not believe that the rules of induction are empirical statements. (Neither, incidentally, does Lewis.) What I believe is that the rules of induction are analytic. They constitute the full expansion of the Principle of Induction itself. I have stated that Principle only very generally and roughly; a full statement of it would include the more special rules of sampling.

I do not doubt that there are certain subsidiary "procedural rules" which really are inductive generalizations. If I were to "proceed" on the rule that in certain branches of chemistry it is relatively useless to repeat an experiment more than twice, this would be to proceed, I suppose, on an empirical generalization. I would be acting on the basis of my inductive knowledge that in this branch of chemistry further repetitions are unlikely to reveal errors. But such a rule ought not to be called a rule of induction. It is established *on* induction, but is not a rule *of* induction. Rather, it is like the procedural rule that if you want to light the Bunsen burner when you get to the laboratory in the morning you had better take a match with you. This is an empirical generalization which can be acted upon, and in that sense it is a "procedural rule"; but it is not a rule of induction. And the rules of induction are analytic.

(3) Miss Brodbeck says that "nothing follows from a tautology." I am sure she would quickly accept the qualification "except other tautologies." Now on my view a specific probability statement is, given the data, a tautology. Hence there is nothing wrong with my justifying such tautologies by deducing them from a more general tautology, namely, from the Principle of Induction itself.

The whole question is just this: what is it you *want* to make "follow" from the Principle of Induction? That is, what do you want to deduce from it? If you want to deduce an empirical statement from it, then obviously you cannot suppose the Principle of Induction to be itself tautologous.

But my whole claim is that as soon as one thinks clearly about the problem one realizes that one does not, and could not, want to deduce empirical predictions from the Principle of Induction. Certainly one does not want to deduce from the Principle of Induction plus the available data the statement "It will rain tomorrow." For then, supposing that I know the Principle of Induction with certainty and supposing that I also know my data with practical certainty (however inadequate these data may be), it would follow by ordinary syllogistic reasoning that I know with practical certainty that it will rain tomorrow.

Well then, if it isn't tomorrow's rain that we want to deduce from the Principle of Induction, what do we want to deduce? The only thing we could want to deduce is the "probability" of tomorrow's rain. And however you interpret the statement "It is probable that it will rain tomorrow," it turns out to be a non-empirical statement. Probabilities are not observable facts which can be predicted. They don't "happen" or "occur"; they are not "events." This is clearly the case if we interpret probability, as I have done, as something which attaches to beliefs or statements. But it is also the case even if one interprets a probability as some sort of an "objective fact." Because even if there were some sort of objective state of affairs, distinct from event *e*, which could be called "the probability of *e*," this state of affairs would not itself be an event. It would have to be regarded as some sort of "eternal fact"—the sort of thing that metaphysicians try to describe in non-empirical statements. So in either case we are not trying to deduce any empirical statement from the Principle of Induction; we are simply trying to deduce what Miss Brodbeck chooses to call tautologies. And tautologies do follow from tautologies.

Perhaps this is the place to mention, also, Miss Brodbeck's statement that, on my interpretation, 'degree of confirmation' names a logical relation between the hypothesis ('P is probable') and the evidence ('On Data D'). This is not quite an accurate statement of my view. On my view, the logical relation between 'Data D' and 'On Data D, P has degree of confirmation a/b' is not the relation of degree of confirmation. It is the relation of strict equivalence. 'Degree of confirmation a/b on Data D' is a predicate which can be attached to P itself. It does not directly name any "relation" in which P stands, although the whole statement 'On Data D, P has degree of confirmation a/b' is translatable into a statement which is about the relations between P and certain other true statements (the data).

(4) The last point brings me to the central objection which Miss Brodbeck raises. This is her claim that I have failed to "justify"

anything worth justifying. I think she has seen the issue here clearly and that she is quite correct in coming back to it again and again in various forms. For it is crucial. But although I feel that she raises the proper question, I still believe that my position on this question is essentially correct.

Miss Brodbeck points out that what I have claimed is that the statement 'On Data D, P is probable' is analytically true; and that I have made this claim on the basis of a definition in use of this whole expression. She then says that since the definition offered is a definition of the *whole* statement above, it tells us nothing about P itself, since P is only a component of the whole statement. And since the problem of induction is precisely "the problem of the justification for saying more than what has been observed, for asserting P itself," she concludes that my justification of 'On Data D, P is probable' has nothing to do with what is meant by the problem of induction. The problem of induction is concerned with P. I have justified 'P is probable.'

Now I certainly agree with her that the problem of induction is the problem of justifying P (where P is any predictive statement). Consequently, if the whole statement 'On Data D, P is probable' does not say anything at all about P, and therefore necessarily fails in any sense to "justify" P, then I must agree with her that everything I have said is quite irrelevant to the problem of induction.

Let us take a case. Let us take, not Miss Brodbeck's generalization that cats meow, but the more specifically predictive statement that Mehitabel will meow sometime tomorrow. What I have claimed is that the statement 'On Data D, the statement "Mehitabel will meow sometime tomorrow" is probable'—that this whole statement is analytic. And I have said that the reason it is analytic is that the latter part of it—' "Mehitabel will meow sometime tomorrow" is probable (On data D)'—is simply another way of saying Data D.

I think I can see why Miss Brodbeck thinks that all of this misses the point. For the sentence I am trying to "justify"—'Mehitabel will meow sometime tomorrow'—says something about Mehitabel. But the sentence I offer as a justification of it—'It is probable that Mehitabel will meow sometime tomorrow'—says (in my interpretation) simply that past cats have usually meowed. It would appear, therefore, that the statement 'It is probable that Mehitabel will meow tomorrow' says nothing about Mehitabel and that, indeed, it says nothing even about our *belief* that Mehitabel will meow tomorrow. It seems to say something about past cats—something which is, as far as I have shown, quite independent of either Mehitabel or our belief about Mehitabel.

But I believe I can show that the statement 'Mehitabel will prob-

ably meow sometime tomorrow' *does* say something about our belief that Mehitabel will meow sometime tomorrow.

But I want first to agree with Miss Brodbeck that the statement 'Mehitabel will probably meow tomorrow' (interpreted as I interpret it) says nothing whatsoever about Mehitabel. I shall claim that it does say something about our *belief* about Mehitabel. But it does not say anything about the thing that our belief is about. Indeed, I do not see how, on any theory of probability statements, we could suppose that the statement 'Mehitabel will probably meow tomorrow' says anything about Mehitabel. There are only two things to be said about Mehitabel in this context—either that she will meow or that she won't. And since the statement that she probably will doesn't say either of these things, it follows that it does not say anything about Mehitabel.

I repeat once more that the problem of induction is not the problem of finding a connection between past facts and future facts (such as Mehitabel's meowing). In the last paragraph of her comments Miss Brodbeck says that the problem is about the "relationship between the observed and the unobserved." But I am sure she is wrong about this. The problem of induction is the problem of finding a connection between the observed and *beliefs* about the unobserved.

Let us now deal directly with the crux of the objection. Granted that 'On Data D, Mehitabel will probably meow sometime tomorrow' says nothing about Mehitabel, nonetheless, if it is to justify our empirical belief that Mehitabel will meow sometime tomorrow, it must say something about that *belief*. What does it say about that belief?

Suppose that D consists of three bits of knowledge, Q, R, and S (these are bits of knowledge about the past habits of cats). Supposing this, then the statement 'On Data D, Mehitabel will probably meow sometime tomorrow' can be expanded into a more explicit form. It becomes '(a) Q and R and S; (b) Q and R and S are data for "Mehitabel will meow sometime tomorrow"; and (c) on the basis of (a) and (b), Mehitabel will probably meow sometime tomorrow.'

Part (a) of this expansion simply states what we know about the past habits of cats. Part (c) is strictly equivalent to parts (a) and (b) taken together. What does (b) say?

It is part (b), I think, that tells us something about the empirical belief P (that Mehitabel will meow sometime tomorrow). When part (b) says that Q and R and S are "data" for P it is telling us that P resembles Q and R and S in certain ways—that P is the same sort of belief as Q and R and S. More particularly, it tells us that

P asserts the same property of the same sort of entity (a cat) as Q and R and S do. That is, it tells us that P and Q and R and S are all beliefs to the effect that an entity defined as belonging to a certain reference class (the class of cats) possesses a certain quaesitum property (the property of meowing at least once a day).

Thus, to believe 'On Data D, P is probable' is to believe *both* that P employs the same functions as Q and R and S, *and* that Q and R and S have been found to be true. Hence, when we say "On Data D," we are not only asserting Q and R and S. We are also saying that P resembles them in the way required for them to be *data* for P.

More precisely: when we say 'On data ϕa, ϕb and ϕc, it is probable that ϕd' we are saying 'ϕd is a belief that a ψ is a ϕ, and ϕa, ϕb, and ϕc are also beliefs that a ψ is a ϕ, and ϕa, ϕb, and ϕc are true.' And this *does* say some thing about ϕd. The reference to ϕd is included in saying that D is a set of facts which are data for ϕd.

I should add that I was not led to the above analysis until I had had the benefit of Miss Brodbeck's discerning criticism. Everything I said in my original paper stands. To say 'On Data D, P is probable' is simply another way of saying 'Data D'. But 'Data D' includes a reference to P; and this I did not see until after I had reflected on Miss Brodbeck's criticism.

(5) Miss Brodbeck ends her criticism by pointing out that on my interpretation the Principle of Induction no longer reads 'If uniformities in the past, then probably the same uniformities in the future' but rather 'If uniformities in the past, then uniformities in the past.' And she then remarks that "merely to repeat that the observed facts are what they are leaves the mystery . . . right where it was."

This sounds damaging. It sounds as if I had produced an even more insignificant tautology than I thought I had. But I suspect the argument is two-edged.

For turn the two statements above around—as can be done, since, in my view, they are equivalent. I can then say that, for me, the obvious statement 'If uniformities in the past then uniformities in the past' strictly implies the statement 'If uniformities in the past, then probably the same uniformities in the future.' And this does not sound so trivial. Indeed, it sounds suspiciously like a justification of our belief in future uniformities—a justification which appeals to nothing more mysterious than the fact that the past has been uniform.

(6) Of course, the issue is whether, when we say 'probably these uniformities in the future', we are saying any more than 'the same uniformities in the past'. I claim we are not. Consequently I do

not have to prove a "connection" between 'Cats always have meowed' and 'Mehitabel (a cat) probably will meow.' For these are two ways of saying the same thing.

Is there some non-analytic sense of 'probable' in terms of which we could say that 'Mehitabel (a cat) will probably meow' goes beyond 'Cats always have meowed', thus posing a problem of "connecting" these two?

Apparently Lewis, to whose analysis mine is in many ways very similar, thinks that there is. And apparently Miss Brodbeck thinks so too.

Lewis, having said that 'On Data D, P is probable' is analytic, then proceeds to become disturbed, just as Miss Brodbeck does, because this statement fails to go beyond past facts. It says nothing about the empirical belief P for which we are seeking justification. We do not, to be sure, want to justify the assertion of P as certain. But we do want to justify the assertion of it as probable—in some sense of 'probable' that does not just turn our attention back to the past. Lewis says that to assert the analytic statement 'On Data D, P is probable' is not the same as to assert the predictive statement P-probably.

On the basis of this distinction Lewis appears to conclude that it is somehow necessary to *assume* induction, not in order to justify 'P is probable', but in order to connect 'P is probable' (the evidence) with 'P-probably' (the empirical prediction).[1]

Lewis confesses that this further sense of 'probable', which occurs in the empirical statement 'P-probably', is as ineffable and irreducible as the sense of futurity itself (*An Analysis of Knowledge and Valuation*, pp. 316–320). I confess I cannot find this further sense.

If Lewis means simply that I often entertain my beliefs with a degree of hesitancy, then I know what he means. And I have no doubt that we often use the term 'probably' to express such a psychological state of indecision.

But, so used, probability seems to be a fact of psychology, not of epistemology, and to be quite irrevelant to the problem of induction. You can give a *causal explanation* of a state of uncertainty. But you cannot give an *epistemological justification* of a state of uncer-

[1] I am not *sure* that Lewis' final conclusion is that induction must be assumed. Professor Henle, in his review of *An Analysis of Knowledge and Valuation*, interprets him this way; and the interpretation seems as good as any. However, there are some things Lewis says which would indicate that he is still holding to the view of *Mind and the World-Order*; and some other things he says that would indicate that perhaps he is holding a view not too dissimilar from mine.

tainty any more than you can give an epistemological justification of what Miss Brodbeck refers to as "the interesting habit men have of making inferences from the observed to the unobserved." There are causal explanations of those mental habits and states which we sometimes express by the term 'probably'. And there are epistemological justifications of probability judgments in my sense of 'probability'. This justification consists of deriving them analytically from past experience. But I cannot see any third thing that can be asked for.

I end with a quotation from Hume—not from the Hume who was self-consciously skeptical about induction, but from the Hume who put forward a positive philosophy to the effect that, in the problem of induction, there arise only the two questions I have noted above: What are the past facts? And, why, psychologically, do men regulate their beliefs by past experience? This quotation expresses my view of the matter.

> Shou'd it be said, that tho' in an opposition of chances 'tis impossible to determine with *certainty*, on which side the event will fall, yet we can pronounce with certainty, that 'tis more likely and probable, 'twill be on that side where there is a superior number of chances, than where there is an inferior: Shou'd this be said, I wou'd ask, what is here meant by *likelihood and probability?* The likelihood and probability of chances is a superior number of equal chances; and consequently when we say 'tis likely the event will fall on the side, which is superior, rather than on the inferior, we do no more than affirm, that where there is a superior number of chances there is actually a superior, and where there is an inferior there is an inferior; which are identical propositions, and of no consequence. The question is, by what means a superior number of equal chances operates upon the mind, and produces belief or assent; since it appears, that 'tis neither by arguments deriv'd from demonstration, nor from probability. [*Treatise*, Bk. I, Pt. III, Section 11.]

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BOOK REVIEWS

Toward a General Theory of Human Judgment. JUSTUS BUCHLER. New York: Columbia University Press, 1951. viii, 176 p. $2.75.

Professor Buchler tells us in the preface of this book that it is an attempt "to lay a conceptual foundation for the understanding of such phenomena as symbolism and language, meaning and representation, communication and method." He goes on to say, "The present approach uses as expedient the notion of utterance or judgment: the principal problem is to define the generic con-

ditions and properties of the human product. By a product I understand anything at all (any instance of making, doing, or saying) that issues from human life or human relationship, and I conceive of every product as a judgment—in the sense which the sequel describes."

This general theory (and the word "general" is to be taken very literally) is developed in a series of six chapters, the titles of which indicate the six principal ideas which Mr. Buchler is concerned with: Proception, Communication, Compulsion, Convention, Perspective, and Validation. The term "proception" is introduced as a new term on page 4. Professor Buchler has discussed the inadequacy and possibly even the misleading character of the terms "experience" and "behavior." He introduces his new term as follows: "The interplay of the human individual's activities and dimensions, their unitary direction, constitutes a process which I shall call *proception.* The term is designed to suggest a moving union of seeking and receiving, of forward propulsion and patient absorption. Proception is the composite, directed activity of the individual. Any instance of his functioning, any event in his history enters into the proceptive direction. Reciprocally, and perhaps more significantly, the way that an individual will act at any time, and the way in which his intellectual and moral character will be modified, depends on his proceptive direction. Proception is the process in which a man's whole self is summed up or represented."

Later on (page 7) Mr. Buchler says, "A procept is anything that is a property of the individual, that happens to him, that affects or characterizes him in any way at all, so long as it relates to him as a proceiver (as an identifiable and cumulative individual) and not as a mere entity in the cosmic maelstrom."

The notions of the *gross* proceptive domain, the *floating* proceptive domain, and the *imminent* proceptive domain are then introduced. A further characterization of the term "procept" is given on page 11: "A procept is whatever natural complex can be identified or discriminated in the life process of the individual. An object can be the same object for any number of individuals; a procept, by definition, is that object as uniquely modified by relation to a given individual."

The first chapter, in which this notion of proception and its related terms are introduced, is so compactly written that it is impossible to summarize it adequately. On page 17, however, we begin to understand how Mr. Buchler intends to use this term with relation to other philosophical ideas with which we are already familiar. He writes, "There are two fundamental and

correlative dimensions in the proceptive direction: manipulation and assimilation." When Mr. Buchler tells us later on that John Dewey, in analyzing man's relationship to his world, overemphasizes that aspect of proception which he calls "manipulation," and underemphasizes the aspect which he calls "assimilation," we begin to understand a little more clearly what the introduction of this new term is supposed to accomplish. The present reviewer, who has no desire to oversimplify Mr. Buchler's contribution, sees his general problem as follows: The process philosophies of the past fifty years have had great difficulty in finding an adequate vocabulary for their basic notions. Various alternatives have been open to them. Either they have taken familiar terms, as Bergson did with the term "time," and given them new or strange meanings, in which case they have laid themselves open to serious misinterpretation; or, like Whitehead, they have invented new terms such as "prehension," "occasions of experience," "actual entity"; or they have endeavored to develop their notions without the use of any new terms, in which case they have had to spend much of their time pointing out that they are using the traditional terms in a new sense.

This third alternative seems to be the one which Mr. Dewey used in most of his writings. The result is that much of Dewey's writing has a polemical character. If one endeavors to state Mr. Dewey's position in a simple expository manner without contrasting it continually with the background of Kant and the British empiricists against which he is revolting, one very quickly finds that Dewey has not provided a vocabulary of his own in which these ideas can be simply and precisely stated.

Thus it appears that one of the problems which Mr. Buchler is trying to solve is a method of stating a process theory of human behavior, by developing a vocabulary which will be adequate for this purpose. I do not mean to suggest that he is simply trying to restate Dewey in a new vocabulary, for it is quite clear that he has many criticisms of Dewey's position. One virtue of his new vocabulary, however, would be that it would enable him both to state and to criticize Dewey's theories.

Mr. Buchler thus avoids the polemical character of much of Dewey's writings. On the other hand, this very fact makes it sometimes difficult for the reader to understand exactly what Mr. Buchler means at any given point. Had he been a little bit kinder to his reader he would have contrasted his position more frequently with familiar classical philosophies, or he would have given more examples. Mr. Buchler occasionally drops into an epigrammatic style of writing which is reminiscent of Whitehead. It may be

merely the fact that Whitehead has been around longer that makes his epigrams seem more appropriate than Mr. Buchler's do.

At the beginning of the second chapter Mr. Buchler introduces the notion of communication, which he says presupposes and is presupposed by proception. "Without communication proception would be little more than protoplasmic endurance." This notion in turn presupposes the notion of mutuality or community, which Mr. Buchler goes on to develop at considerable length.

In this second chapter Mr. Buchler has much that is of interest to say. However, Mr. Buchler, writing as a naturalist, does not always make it clear at what level of metaphysical analysis he is talking. The general topic of the book is the topic of human products or judgments. If the discussion of communication or community is to be considered only at the level of humans, then it would seem that Mr. Buchler should distinguish in this chapter what characterizes human communication from other forms of intercommunication of natural entities. He seems to go beneath the level of human intercommunication in this chapter, but the level of analysis is not always made completely clear.

In chapters three and four the notions of compulsion and convention are considered as "the two principal modes in which the proceiver judges relative to the world and to the products of communication" (page 58). The contrast between the two modes is explained frequently. Possibly the following passage will make the contrast clear. "Compulsion obtains where a judgment cannot be regarded as determined by decision; convention, in any other case—that is, in so far as a judgment can be regarded as determined by decision" (page 90). As an illustration of what Mr. Buchler means by compulsion, we might quote the famous sentence from William James, "Every sentence has been forged in the teeth of the stubborn and irreducible facts." The notion of convention, however, Mr. Buchler generalizes from the familiar meanings of that term. His discussion has much of value in it.

The final chapters on perspective and validation have a more familiar ring to them. The notion of perspective, for example, seems reminiscent of the element of subjective form in Whitehead's "occasions of experience." The notion of validation is a generalization, as Professor Buchler tells us, of "the elimination of doubt (Peirce), the resolution of a problem (Dewey), the mitigation of blocked conduct (Mead)."

One of the interesting features of this book is its attempt to apply the same methods of analysis to the three types of human products which Aristotle analyzed, and which Mr. Buchler calls "the assertive, active, and exhibitive modes of judgment." In

other words, human decisions respecting action, works of art, and theoretical judgments are all covered by this mode of analysis, and their specific differences considered.

In his preface Mr. Buchler promises that there will be further books to develop these notions in greater detail. It seems to this reviewer that we will have to await these further books before we can give an adequate evaluation of Mr. Buchler's contribution. He is working in an extremely interesting field. Just as Dewey hoped to apply the same methods of analysis to the production of works of art, moral judgments, and scientific hypotheses, so Mr. Buchler hopes to develop a conceptual machinery which will be adequate for the analysis of these three major types of human products. Similarly we are reminded of Mr. Whitehead's dictum that the subject-object relationship can be taken as the basic relationship for a naturalistic metaphysics, provided that it is not confined to the knower-known relationship.

As yet, however, Mr. Buchler's theories seem to be suspended in mid-air. Until he uses his new vocabulary and his new concepts to clarify some specific problem in detail, it will not be possible to determine whether he has made an important advance over his predecessors in the field of process philosophy.

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Ethics and the History of Philosophy. Selected Essays. C. D. BROAD. New York: The Humanities Press; London: Routledge & Kegan Paul, 1952. xiii, 274 p. (International Library of Psychology, Philosophy and Scientific Method.) $4.50.

The subtitle of this latest volume of Professor Broad's gives one a more accurate clue to its contents than the title itself. The selected essays are lectures and papers written on various occasions, such as the tercentenary of Locke's birth, the tercentenary of Bacon's death, the author's visit to Uppsala, where he was naturally enough asked to speak to the Philosophical Faculty, the publishing of the so-called Oxford Oath. One must not, then, expect to find that ethics has much to do with the history of philosophy; the "and" in the title is a simple connective. But since everything which Mr. Broad writes has a certain interest, it may be worth while to indicate precisely what the contents of this miscellany are. There are five biographical essays, on Newton, Locke, Sidgwick, McTaggart, and W. E. Johnson, the logician. There are three papers on the philosophy of science, of which the first, on Bacon's philosophy, is particularly interesting and the one on

Descartes the least original. There are four papers on Ethics, accurately enough described by their titles: "Determinism, Indeterminism, and Libertarianism," "Egoism as a Theory of Human Motives," "Ought We to Fight for Our Country in the Next War?" and "Conscience and Conscientious Action." It is perhaps needless to say that these four essays show the same precision of statement and the same meticulous analysis of meanings and possible meanings which we are accustomed to find in the works of this author.

It is hardly likely that anyone reading these last four papers would use the terms "determinism," "indeterminism," and "egoism" with the insouciance with which they have been used in the past. Again, Mr. Broad's analysis of the meaning of the word "conscience" will surely prove illuminating to any philosopher inclined to use the term. Moreover, the great value of such analyses may be shown in the discouragement which they may give to those who hope to find on this earth exact ontological equivalents for the meanings of such terms. That is, it is clear that no one will be fully conscientious—and Mr. Broad admits this— if by "fully conscientious" one means (1) that the agent "has reflected on the situation, the action, and the alternatives to it, in order to discover what is the right course," and (2) "he has decided that . . . the action in question is probably the most right or the least wrong of all those which are open to him," and (3) "his belief that the action has this moral characteristic, together with his desire to do what is right as such, was either (a) the *only* motive-component for doing it, or (b) a *sufficient and necessary* motive-component for doing it." To fulfill all these conditions would be next to impossible in any real case, but the statement has the merit of clearly defining the ideal case which we may presumably approach as closely as we wish. Moreover, Mr. Broad also admits that conscientious action may be mistaken, since obviously there are many grounds for error in making the series of judgments which would be required for such action.

Now it would be folly to object to an analyst's analysing terms, though how one knows when one's analysis is correct has escaped this reviewer's notice. But on the other hand, the great weakness of logical analysis is that by its very nature it must take a term or concept at some point in its history and deal with that, rather than consider the origin of the term or the historical roots of the concept. If a term or concept were a static something whose history was simply a series of discrete stages, like beads on a string, then the resultant analyses might be used as correctives of anyone's conduct at any time. But unfortunately the various past

meanings of a term hang about it or survive, if the phrase is preferable, in the form of rhetorical connotations, emotional auras, sentimental associations, which are often more powerful in making the term an impetus to action than its denotation. Thus it is not enough to point out what a term means in 1952 among philosophers, or the only intelligible definition which can be given at that date. For if one does simply that, then the force of the term or concept as an instrument seems inexplicable. One must of course discover as precisely as possible what a person is talking about when one is arguing with him. But when one wants to know why he utilizes a term in a certain way, then it may be much more important to see how ambiguity itself turns into a powerful force and the ambiguity can be best understood through historical semantics. I am only too happy to admit that frequently a very obscure term may be a powerful social instrument and that its power would disappear if the obscurities were dissipated. It is also correct to admit that one of the duties of philosophers is to clarify meanings. But when one is studying a subject—like human conduct—where the impulsion to act may lie in obscurity rather than in clarity, then it is at least questionable whether one has solved any real problems of conduct by stating what certain terms should mean if the agent were rational and had read one's works.

Aside from all that, how does one know whether one's refinement and restatement of meanings are right? Much of metaphysics consists admittedly in making the conclusions of common sense intelligible. Is the science of ethics a similar undertaking, in that it makes the prejudices of an author intelligible? Mr. Broad is delightfully frank in stating his prejudices. Thus he has a preference for "the upper middle-classes" (p. 45). He believes that most men "cannot be happy, and perhaps they cannot be spiritually healthy, without believing nonsense with a degree of passionate conviction proportional to the lack of evidence for it" (p. 47). He seems to have a strong abhorrence of social reformers (p. 69) and prefers the "golden afternoon of Victorian civilization" to their "new and nasty Jerusalems." One could go on listing such statements, many of which would meet with one's hearty approval but none of which would seem to follow from any ethical principles enunciated by Mr. Broad. Nor does Mr. Broad maintain that it is the business of the ethicist to justify the conclusions of common sense, for which, he says (p. 230), he has no "superstitious reverence." For that matter, it is doubtful whether they would follow from anything in his *Five Types of Ethical Theory*. What, then, is the relation of moral taste to ethical theory? If the question is important, then the further

question must be raised of whether it can be answered by the logical analysis of commonly used terms.

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NEW BOOKS

(Books listed here may be reviewed later)

ARISTOTLE: *Aristotle's Metaphysics.* Newly Translated as a Postscript to Natural Science, with an Analytical Index of Technical Terms, by Richard Hope. New York: Columbia Univ. Press, 1952. xvii, 394 p. $5.00.

ARISTOTLE: *The Works of Aristotle.* Translated into English under the Editorship of Sir David Ross. Vol. XII. *Select Fragments.* Oxford: Clarendon Press, 1952. xii, 162 p. $3.00.

BREWER, JOHN M.: *Wellsprings of Democracy.* New York: Philosophical Library [1952]. xii, 232 p. $4.50.

CONANT, JAMES B.: *Modern Science and Modern Man.* New York: Columbia University Press, 1952. 111 p. (Bampton Lectures in America, Number 5, Delivered at Columbia University, 1952.) $2.25.

DINGLE, HERBERT: *The Scientific Adventure; Essays in the History and Philosophy of Science.* London: Sir Isaac Pitman & Sons, Ltd. [1952]. ix, 372 p. 30s. ($6.75 at the British Book Centre, New York).

FERRATER MORA, JOSÉ: *Diccionario de filosofía.* [Tercera edición.] Buenos Aires: Editorial Sudamericana [1951]. 1047 p. $150.- m/arg.

The third edition of an excellent dictionary which South American teachers and students of philosophy have found extremely useful. Double in size to the second edition (1944) and triple in size to the first edition (1941). Three columns to a page. Extensive and up-to-date bibliographies. North American readers will find the articles on South American philosophers useful.

FOUÉRÉ, RENÉ: *Krishnamurti, the Man and His Teaching.* Translated from the French. Bombay: Chetana [1952]. 73 p. (The Inter-Continental Library.) Rs. 2 ($1.00).

FRANKLIN, BENJAMIN: *The Autobiography and Selections from His Other Writings.* Edited, with an Introduction, by Herbert W. Schneider. New York: Liberal Arts Press [1952]. xx, 218 p. (The American Heritage Series, Number Two.) Paper $0.75.

Contains the Autobiography; "Humanitarian Essays," including "Observations on War and Privateering," "On the Criminal Laws," "A

Parable Against Persecution," "A Letter on Religious Tolerance," "On the Slave Trade," "On the Freedom of the Press," "A New Mode of Lending Money," and "On Bringing the Conveniences of Life to New Zealand"; "Information to Those Who Would Remove to America"; and "The Way to Wealth." In the Autobiography, Max Farrand's "Fair Copy" is followed with minor changes.

KEGLEY, CHARLES W., and BRETALL, ROBERT W., eds.: *The Theology of Paul Tillich.* New York: Macmillan, 1952. xiv, 370 p. (The Library of Living Theology, Volume I.) $5.50.

KLUBERTANZ, GEORGE P., S. J.: *The Discursive Power; Sources and Doctrine of the* Vis Cogitativa *According to St. Thomas Aquinas.* Saint Louis, Mo.: The Modern Schoolman [1952]. viii, 353 p.

KOHLER, FRED: *Evolution and Human Destiny.* New York: Philosophical Library [1952]. 120 p. $2.75.

LOCKE, JOHN: *The Second Treatise of Government.* Edited with an Introduction by Thomas P. Peardon. New York: Liberal Arts Press [1952]. xxviii, 139 p. (The Library of Liberal Arts, Number Thirty-One.) Paper $0.75.

MASLOW, PAUL: *Powers of the Mind.* Volume II of *The Life Science.* Brooklyn, N. Y., 16 Court Street: Paul Maslow, 1952. Pp. 135–288. $3.50.

MAYO, BERNARD: *The Logic of Personality.* London: Jonathan Cape [1952]. 188 p. 10s. 6d.

NORTHROP, F. S. C.: *The Taming of the Nations; a Study of the Cultural Bases of International Policy.* New York: Macmillan, 1952. xii, 362 p. $5.00.

Chapter-headings: 1. The Nature of the Problem. 2. Pearl Harbor, Hiroshima and Korea. 3. India's Response to Korea. 4. Gandhi's Way to Peace. 5. The Contemporary Mind of Islam and of Asia. 6. The Domestic Policy and Needs of Free India. 7. Asian Resurgence and Western Ways. 8. The Resurgence of Islam. 9. How Can We Defend Free Culture? 10. The Spiritual Foundations of Western Civilization. 11. The Modern Fragmentation of Western Civilization. 12. The Theory and Practice of Soviet Russian Communism. 13. Hiroshima, Korea and an Effective World Law. 14. Two Paths to World Law. 15. The Cultural Basis of Foreign Policy. 16. From Philadelphia to Cosmopolis.

PLEBE, ARMANDO: *Hegel, filosofo della storia.* Torino: Edizioni di "Filosofia" [1952]. vii, 148 p. (Studi e ricerche di storia della filosofia. VII.) L. 1000.

Problèmes actuels de la phénoménologie. Textes de P. Thévenaz, H. J. Pos, P. Ricoeur, E. Fink, M. Merleau-Ponty, J. Wahl. Édités par H. L. Van Breda, O. F. M. [Actes du Colloque international de Phénoménologie. Bruxelles, Avril 1951.] Desclée De Brouwer [1952]. 165 p. (Textes et études philosophiques.) 480 fr.

ROBACK, A. A.: *History of American Psychology.* New York: Library Publishers [1952]. xiv, 426 p. $6.00.

Contains, in Part I, materials on the history of psychology in America before its separation from philosophy; in Part II, discussion of the early years of psychology's independence; in Part III, summary statements of the impact of the various "schools" on psychology in America; in Part IV, an exceedingly cursory survey of the present situation in psychology. The materials of the first part, while not as complete as those in Jay Wharton Fay's *American Psychology Before William James*, mark an advance over the earlier book in organization and interpretation, and suggest the steps toward separation from philosophy that were implicit in the psychological developments. The philosophic interest of the later sections of the book is slight, even where, as in the case of William James, James M. Baldwin, and John Dewey, there would be justification for dwelling on the implications of their psychologies for their general philosophic positions.

ROMERO, FRANCISCO: *Teoría del hombre.* Buenos Aires: Editorial Losada, S. A. [1952]. 362 p. (Biblioteca filosófica, publicada bajo la dirección de Francisco Romero.)

ROSSER, J. BARKLEY, and Turquette, Atwell R.: *Many-Valued Logics.* Amsterdam: North-Holland Publishing Co., 1952. 124 p. (Studies in Logic and the Foundations of Mathematics; L. E. J. Brouwer, E. W. Beth, A. Heyting, Editors.) $3.25.

This latest addition to the "Studies in Logic and the Foundations of Mathematics" is a systematic presentation of many-valued logical calculi, and contains axiomatizations for many-valued statement calculi and predicate calculi of the first (but not higher) order. No interpretations or applications of the calculi are indicated, although occasional suggestions occur for interpreting them—primarily for the sake of motivating the constructions. Indeed, the authors believe that since many-valued calculi have not as yet been developed to the point of incorporating a theory of numbers, recent attempts to find applications for them are premature. They are nevertheless of the opinion that despite the great complexity of even the more elementary portions of such calculi, there is a reasonably fair chance that fully developed many-valued "logics" may find significant applications and that continued research in this domain is worth the effort. The monograph assumes no previous knowledge of the subject, though it does take for granted familiarity with the two-valued lower functional calculus. The final chapter contains a list of still unsolved problems in the construction of many-valued systems.

ROYCE, JOSIAH: *The Religious Philosophy of Josiah Royce.* Edited, with an Introductory Essay, by Stuart Gerry Brown. Syracuse Univ. Press, 1952. 239 p. $3.00.

Contains "The Possibility of Error," from *The Religious Aspect of Philosophy;* "Individuality and Freedom" and "The Temporal and the Eternal," from *The World and the Individual;* "The Conception of Immortality" (1900); "Loyalty and Religion," from *The Philosophy of Loyalty;* and "The Idea of the Universal Community," "The Moral Bur-

den of the Individual," "The Realm of Grace," "Time and Guilt," and "Atonement," from *The Problem of Christianity.*

RYAN, JOHN K.: *Philosophical Studies in Honor of the Very Reverend Ignatius Smith, O. P.* Edited by John K. Ryan. Westminster, Md.: Newman Press, 1952. x, 316 p. $5.00.

John K. Ryan: Introduction. *Joseph M. Marling:* The Dialectical Character of Scientific Knowledge. *Gerald Benkert:* Thomistic Philosophy and International Society. *Jules A. Baisnée:* St. Thomas Aquinas's Proofs of the Existence of God Presented in Their Chronological Order. *Rudolf Allers:* Ens et Unum Convertuntur. *Joseph B. McAllister:* Chance in Aristotle and Aquinas. *John T. Noonan:* The Protestant Philosophy of John Locke. *Francis X. Meehan:* Lux in Spiritualibus According to the Mind of St. Thomas Aquinas. *Owen Bennett:* Existence and the First Principles According to St. Thomas Aquinas. *James Collins:* Scheler's Transition from Catholicism to Pantheism. *J. A. McWilliams:* Action Does Not Change the Agent. *William J. McDonald:* Toward a Philosophy of Economics. *Allan B. Wolter:* The Recognition of Miracles. *Jean R. Rosenberg:* Freedom in the Philosophy of Kant. *Brendan F. Brown:* The Law of Contracts and the Natural Law. *George C. Reilly:* The Dynamics of Moral Conduct. *Edward A. Maziarz:* Number Freedom.

SCHILPP, PAUL ARTHUR, ed.: *The Philosophy of Sarvepalli Radhakrishnan.* New York: Tudor Publishing Co. [1952]. xiv, 883 p. (The Library of Living Philosophers.) $7.50.

SCHNEIDER, HERBERT WALLACE: *Religion in 20th Century America.* Cambridge, Mass.: Harvard Univ. Press, 1952. x, 244 p. $4.25.

SOOTHILL, WILLIAM EDWARD: *The Hall of Light; a Study of Early Chinese Kingship.* Edited by his daughter Lady Hosie . . . and G. F. Hudson. New York: Philosophical Library [1952]. xxiv, 289 p. $7.50.

THIBON, GUSTAVE: *What God Has Joined Together; an Essay on Love.* Chicago: Henry Regnery Co., 1952. viii, 192 p. $2.50.

NOTES AND NEWS

The Conference on Methods in Philosophy and the Sciences will hold its 32nd Semi-Annual Meeting at the New School for Social Research, New York City, on Sunday, November 23, 1952.

The program is as follows: Morning Session: "The Three Faces of Entropy," by Brockway McMillan, and "The Negentropy Principle of Information," by Leon Brillouin; Afternoon Session: "In Memory of John Dewey," by Sidney Hook; "In Memory of George Santayana," by Horace M. Kallen; "Time's Arrow and Man," by Harold F. Blum; and "Time as Affective-Conative Category," by Wilmon H. Sheldon.

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